

CORRESPONDENCE

TO THE EDITOR OF *Philosophy*

SIR,

In No. 42, April 1936, of your journal, p. 230, I find an interesting review of my book *Wahrscheinlichkeitslehre*, by A. G. D. Watson. I am very much obliged to your journal and to the reviewer for the kind way my book is spoken of there, and for the profound analysis given. If I allow myself to express some criticism, this will be directed only against some details concerning the development of the discussion of the problem in Germany, as to which, it seems, the reviewer is not sufficiently informed.

Dr. Watson writes: "This work has been largely carried on under the influence of the philosophical school of Carnap, to whom the author expresses his indebtedness." This passage—which by no means may be based on an utterance from my side—reveals a deep-going misunderstanding of the development of our movement of "scientific philosophy" or "logistic empiricism," as we now call it, in Germany. This movement always concentrated round two centres and split accordingly into the Vienna Circle, whose members were Carnap, Schlick, etc., and the Berlin group in which I myself collaborated with some friends. The two groups combined later to common work, marked by the publication of the journal *Erkenntnis*, edited jointly by Carnap and myself, and by the organization of the congresses for scientific philosophy. Our collaboration was based on many common ideas; as to others, we learned from each other—but it never came to a perfect unification of ideas, the differences as to some fundamental problems being too great.

This difference neither disturbed the personal friendship between the groups, nor our collaboration. Only, whoever followed our discussions knows that just the question of probability was the point of bifurcation. Thus, I think, the idea that my theory of probability has been developed under the influence of Carnap's school not only contradicts the historical facts, but will also be rejected by my Vienna friends, who to-day hold a very different opinion on the problem. It may be added that the fundamentals of my theory of knowledge and of probability were developed and published at a time when there was not yet any Vienna school at all. For a nearer exposition of these historical developments, I may refer to my article "Logistic Empiricism in Germany and the Present State of its Problems" in the *Journal of Philosophy*, March 12, 1936, p. 747.

Allow me to add some words concerning some critical remarks of the review. Dr. Watson objects that the mathematical concept of infinity used in the mathematical theory of probability does not play any rôle as to physics. This idea does not contradict my theory. In an article, "Bemerkungen zu Carl Hempels Versuch einer finitistischen Deutung des Wahrscheinlichkeitsbegriffs," *Erkenntnis*, vol. v, 1935, p. 261, I have shown that it is the concept of "practically infinite" which stands at the basis of applied probability, and that my theory is applicable for this concept as well. In another objection, Dr. Watson asks whether there may be a world for which the induction rule leads to no success, whereas other methods such as "oracles" are utilizable. This objection has also been discussed in *Erkenntnis*; I have shown there that such a separation is not possible, that if any method of prophecy exists the induction rule will always lead to the same result. This is exposed in my article "Warum ist die Anwendung der Induktionsregel für uns notwendige Begingung zur Gewinnung von Voraussagen?," *Erkenntnis*, vol. vi, 1936, p. 32. Thus the discussion in *Erkenntnis* has already clarified some objections raised, for good reasons, in the review of Dr. Watson.

Please allow me to repeat that this letter is not to criticize the excellent analysis of Dr. Watson. It is only to direct your attention to some points well known to German readers, but which, on account of the separation by language, seem to have escaped Dr. Watson's investigations.

Yours very sincerely,

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